

Review of U.S. Fish and Wildlife Service species status assessment Bi-state distinct
population segment of Greater Sage-grouse

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The status assessment is an exhaustive review of literature of impacts on Greater Sage-grouse that assesses the case for listing of the Bi-state DPS as threatened or endangered. The key issue, in my view is the declining trend and continuing threats to the PMUs peripheral to the Bodie and south Mono PMUs. The document correctly points out that loss of sage-grouse from more peripheral populations ultimately threatens the larger populations in South Mono and Brodie. I believe this point could be more clearly stated in the Executive summary; it tends to be buried in the main body of the current document. The document would be strengthened by summarizing the key threats to the peripheral PMUs and the extent to which they are being addressed. There are many places where I believe the cited literature doesn't support the point the authors are making. I think these cases weaken the document and I have attempted to point them out. My detailed comments follow.

P. 6, I. 4. Recruitment rates may be low but reproductive rates are relatively high for an avian species the size of sage-grouse.

P. 6. L. 8. Is the trend actually known?

P. 7. Taxonomy. It's not clear in this document how the subspecies recognized by the AOU are related to the two current species or the Bi-state DPS versus other population segments. A description of the subspecies proposed range and morphological similarities to extant sage-grouse units would help clarify this confusion.

P. 8, para. 1. What about Oyler-McCance's work, which is relevant here?

P. 10, para. 3. Sage-grouse in the Bi-state area as well as other parts of Nevada rely on mixed shrub communities (Kolada et al. 2009, Nonne et al. 2013) and nest survival is influenced more by the cover of non sagebrush shrubs than by sagebrush. The importance of this diversity is not reflected in the above text.

P. 10, last sentence. I'm not sure what this sentence means. Does it mean that sage-grouse are faithful to particular areas, as stated in the sentence before.

P. 11, II. 3-7. This statement depends on the assumption that all copulations on leks are observed, which is almost certainly false.

P. 12, I. 9. I would reword to indicate females may travel up to 20 km. Current wording makes it sound like such movements are common but most nests are much closer to leks than 12.5 miles.

P. 12. II. 36-37. Evidence from the western part of SG range indicates that grass cover is less important than it may be elsewhere (Kolada et al. 2009, Gibson et al. 2013).

P. 12, II. 39-41. Kolada, Gibson and unpublished USGS work indicates that in NV shrub cover > 40% can benefit nests. No evidence for the importance of grass height in NV and a number of other studies have been misinterpreted. For example, Holloran et al. (2005) actually demonstrated quite clearly that grass height did not affect nest success of SG in WY (compare successful and unsuccessful nests in their Table 3), despite conclusions to the contrary.

P. 13, Para. 3. Because of the analytical methods used these other studies are likely biased > 25% high relative to estimates in Kolada et al. and Gibson et al.

P. 13, last para. Are these chicks per hen ratios at hatch, fledging or when? I'm not sure they are meaningful without more information about the basis for the estimates.

P. 14, ll. 6-7. In drier areas broods begin moving shortly after hatch and could be beyond 5 km by 3 weeks post hatch.

P. 14, ll. 14-17. I don't believe Casazza et al. differentiated early from late brood habitat.

P. 17, Current Range/. Should be Appendix B.

P. 22. Desert Creek Fales, para 2. It would be helpful to identify main causes of habitat loss here.

P. 25, para. 4. Blomberg et al. (2013) show that male attendance at leks, hence lek counts, vary as a function of precipitation. Consequently, short term trends (< 5 years) cannot be used as indicators of population trend. Thus, trend since 2010 is not informative. In this light, it would be helpful in the current document to provide summaries of all of the lek count data either in the form of graphs or tables.

P. 25, para. 5, ll. 1-2. Garton refers to the Bodie PMU as the Mono Population. It would assist readers to identify the difference in nomenclature in the text here.

P. 37, para. 2, ll. 6-8. This statement requires documentation as many will disagree with it.

P. 37, para. 3, ll. 1-4. I don't disagree but for lands already converted to agriculture the benefit may be decades in the future.

P. 37, para. 4, ll. 8-10. I would caution that Aldridge et al. (2008) does a poor job of predicting persistence in the Bi-state area (see fig. 4).

P. 38., para 38., ll. 6-12. This is an important point but is it documented elsewhere in the document?

I believe the threat goes beyond the problem of fidelity. Late brood rearing habitats typically represent a very small percentage of the total landscape. These habitats are essential to successful recruitment of sage-grouse. Removal of relatively small areas of late brood habitat may eliminate sage-grouse from much larger areas of nesting habitat because remaining late brood habitat may be too far for successfully nesting females to reach with their broods. The result is that no sage-grouse are recruited from these nesting areas.

P. 39, para. 2. I think it is important to be cautious here. The fragmentation argument depends on the demonstration that the structure (e.g., road) prevents movement of sage-grouse between patches of suitable habitat on either side of the barrier. Studies where such effects have been demonstrated are rare. This is not to say such

anthropogenic structures have no effect, it's just that the effect may not be fragmentation.

P. 39, para. 3. Fragmentation is often loosely used and poorly defined. Its use should be restricted to situations where habitats become more isolated from each other because a human activity, e.g., building a road separates one patch of habitat from another. In this context, the anthropogenic features must act to reduce movements between what were previously contiguous areas.

P. 39, para. 3., ll. 7-11. I don't believe it is appropriate to attribute impacts of oil and gas development to fragmentation. There are far more direct causal factors at play, including loss of habitat, human activity, noise, and diseases. It would be difficult to discern the effects of fragmentation itself in the presence of these other factors.

P. 39, para. 3., ll. 11-14. Roads and power lines do not result from fragmentation, they are the cause of it. As per above, both roads and power lines have more direct effects than those resulting from fragmentation. There is ample evidence that sage-grouse in Eureka County cross roads and under power lines.

P. 39, para. 4, ll. 5-8. Again, I would avoid use of the term fragmentation except where it can be demonstrated and focus on the direct effects of human caused change.

P. 39, para. 4, ll. 13-14. I believe the authors are referring to cumulative impacts here.

P. 40, para. 3, l. 6. We're really talking about highways here, as most roads in sagebrush country don't have rest areas.

P. 40, para. 3, l. 11. Oyler-McCance et al. did not report avoidance of roads or that roads impeded movement of sage-grouse. They did cite Oyler-McCance's dissertation as stating that roads have a negative impact on sage-grouse.

P. 40, para. 4, l. 4. Hunting of any kind is unlikely to be important for the bi-state population, given restrictions and closures of legal hunting and the general lack of interest in hunting sage-grouse.

P. 40, para. 6, ll. 1-2. I'm not sure this statement can be substantiated. Sage-grouse are commonly observed along roads in some areas. Restrict to specifics mentioned later in the paragraph.

P. 41, 3rd bullet, A very important limitation of all of these post hoc studies is they do not account for other confounding factors. Roads are not placed randomly on the landscape; they will tend to be in lower elevation, lower gradient sites, etc. Consequently, habitats are also not likely to be randomly distributed with respect to roads. Thus, other factors (e.g., climate change, grazing, fire) could be the actual drivers of declines near roads. This is not to say that roads are not problematic, it's just that post hoc studies are a very weak form of evidence. As an example, I controlled for elevation in the Hall and Haney study and the apparent distance to power line effect on lek persistence disappeared.

P. 41, last para. The most reports from the Falcon-Gondor project are indicate effects of roads on nest success.

P. 42, l.2. Lyon and Anderson suffers from the same limitations as other post hoc studies.

P. 42, fifth bullet. I know it's beyond the scope of this document but the intensity of use of these roads is an important variable. A handful of vehicles during a lekking season seems unlikely to have the same effect as multiple vehicles per day, as might occur in association with mining activities, for example.

P. 43, power lines. Most of these references speculate about potential impacts, rather than providing data. It would be good to clarify which is which, so as not to weaken the integrity of the document.

P. 44, first bullet. Are there any historical data on actual predation rates; if not this becomes a "just so" story.

P. 44, last para., ll. 1-5. We have not observed such avoidance for the Falcon-Gondor transmission line in eastern Nevada.

P. 44, last para., ll. 8-9. My recollection is that Pruett et al. did not have preconstruction data, so we can't be sure the movement patterns they observed did not exist in the absence of the line.

P. 45, para. 1, ll. 1-4. An important limitation of Wisdom et al. is that they could not control for the effects of confounded variables, e.g., roads and power lines, hence it is difficult to infer effects of any individual variables

P. 47, para. 2. It would be good to add a final summary comment on inference about potential impacts of transmission lines.

P. 48, para. 4, ll. 6-8. Did Braun really show that sage-grouse avoid areas near fences?

P. 48. Para. 5. The implication here seems to be that fences cause roads, which I don't believe is correct.

P. 50., para. 4, l. 8. I would substitute population size for carrying capacity; they aren't the same thing.

P. 51, para. 1. As I point out above, there is likely to be confounding between habitat and distance to structures that renders many of these correlational studies questionable. Two examples follow. When we do not control for habitat type/quality we find that female sage-grouse actually prefer to nest nearer the Falcon-Gondor transmission line. Controlling for habitat reverses this pattern. Similarly, we only find an effect of the transmission line on nest success when we control statistically for habitat quality. Such confounding has the potential to create both false positives and false negatives as I show above. In the absence of more detail it is, thus, difficult (impossible?) to interpret these studies.

P. 51, para. 3. This paragraph is somewhat disjointed and I don't think the concluding sentence necessarily follows from the earlier part of the paragraph.

P. 51, last para. Given the uncertainty pointed out above, I believe this is too strong for most of the infrastructure in the Bi-state.

P. 55, para. 1. I have similar concern here to several of the earlier statements. Much of the earlier "evidence" for impacts of infrastructure were really hypotheses that were repeated so frequently that they became accepted as evidence. I believe the same is true of the Connelly reference here. That is not to say that infrastructure and associated human activity may not have impacts--clearly oil and gas development in Wyoming have been demonstrated to have such impacts. I believe that early (unpublished) work on wind energy development is not detecting negative impacts on SG. I refer the authors to a recent paper by Blickley, Blackwood and Patricelli (2012. Conservation Biology 26:461–471) who show experimentally that noise reduced lek attendance by males and could provide a mechanism for human impacts on local SG populations.

P. 56, para. 2, ll. 19-22. I believe Casazza and Kolada have both indicated very limited movement between PMUs. While not desirable, such limited movement would reduce the impact of development between PMUs.

P. 56, para. 3, ll. 6-11. Unfortunately, LeBeau's analyses suffer from the same confounding between distance to facilities and habitat that plagues most such studies, at least for sage grouse. It's pretty clear from examining figures in Lebeau's thesis that wind facilities and associated infrastructure are not randomly placed with respect to availability of habitats: turbines appear to be on ridge tops, while the transmission line is in a valley. Consequently, the additive models Lebeau used to assess impacts cannot differentiate between habitat effects and those associated with habitat. Models including interactions between habitat and distance to facilities are needed to separate habitat from facility effects.

P. 67, last para. We have observed substantially less behavioral response to conifers in eastern NV.

P. 68, para. 2, ll. 14-16. Are these rates of expansion still accurate given drying of climate over the last decade? I asked Weisberg about this and he told me that he is seeing die back in some parts of the Great Basin. He also said Tausch is seeing the same thing.

P. 69, para. 2, ll. 2-4. But drying may reduce rate of P-J expansion.

P. 71, para. 1, ll. 4-7. Is fragmentation really a major cause of loss of sagebrush relative to fire and P-J encroachment?

P. 72, para. 2, ll. 10-12. Do the authors mean abandonment or extirpation? The latter seems more likely and it's important to be precise.

P. 72, para. 3. But isn't it possible that there is a combination of fire size and frequency that is beneficial to sage-grouse because frequent small fires create brood-rearing

habitat, which is used by broods? I believe it is important to distinguish these potential benefits of fire from the clearly negative impacts of stand replacing fires. Some will point to the fire in the Pine Nuts last summer, which removed PJ, as a beneficial fire.

P. 73, para. 1, ll. 5-6. I don't believe the data are published but Kolada reported use of small burned areas by broods.

P. 75, para. 1, ll. 3-5. Clarification needed here. Do the authors mean over the long term. Above, text indicates fire is a relatively high risk. Is this consistent with a reduction in fire frequency?

P. 78, para. 1, ll. 3-5. I think just an increase in frequency (e.g., from once to three times a decade) could shift the recruitment-survival balance and population dynamics from stable to decreasing.

P. 83, para. 2, ll. 10-14. I think just an increase in frequency (e.g., from once to three times a decade) could shift the recruitment-survival balance and population dynamics from stable to decreasing.

P. 84, para 2, ll. 1-9. The Connelly view is out of date. First, life-histories (annual survival and fecundity) of sage-grouse are nearly identical to those of ducks for which harvest in the range of 10% are almost certainly fully compensatory (Peron et al. 2012, Sedinger and Herzog 2012). An important shift in thinking is that individual heterogeneity may provide the mechanism allowing for compensation (Sedinger and Herzog 2012, Lindberg et al. 2013), rather than traditional view that compensation is based entirely on seasonal variation in survival. Finally, Sedinger et al. (2010) the only study to assess process correlation between harvest rate and survival rate in sage-grouse, failed to find the negative correlation, necessary for additive harvest mortality.

P. 84, para 2, last sentence. Actually when you think about it habitat destruction, should reduce carrying capacity, thereby increasing density dependent mortality and the potential for compensation.

P. 84, para. 3, ll. 5-7. Connelly et al. 2000 is seriously flawed and should not be cited with respect to causes of mortality. Their calculations of the timing and causes of mortality were all based on the sample that was originally released in spring rather than the sample alive and available to die during a particular season. Consequently their "estimates" of timing and causes of mortality are highly biased.

P. 84, para. 3, ll. 9-11. While true that lambda is highly sensitive to female survival, Taylor et al. (2012) showed that variation in nest success was far more important to variation in lambda because female survival was much less variable. Sedinger et al. (2011) also showed that harvest rate was not the most important driver of female survival, which was lowest in the bi-state area of Nevada where harvest was closed.

P. 85, para. 1, ll. 10-13. Sedinger and Rotella (2005) showed that apparent responses to harvest regulations in Connelly et al. (2003) could be explained by variation in density; harvest regulations were more liberal where sage-grouse were more dense. Thus, lek dynamics could equally well have been explained by local density rather than

harvest. Blomberg et al. (2013) showed that male attendance on leks was negatively influenced by local density, consistent with a density-dependent explanation for patterns in Connelly et al. (2003). I note that Connelly et al. did not actually measure survival, the demographic rate managers are trying to manipulate through harvest regulations.

P. 86, para. 1, ll. 1-4. I reanalyzed the pattern reported by Gibson using all of the south Mono lek count data and the pattern in Gibson et al. completely disappeared. I conclude that the original pattern was a coincidence associated with small sample size.

P. 86, para. 2, ll. 1-3. CA estimates of fall population size are conservative because they assume 1:1 sex ratio and that 90% of males are counted on leks. Sex ratio more likely 2 females/male and only 85% of males are counted. Harvest is thus < 2% in both areas since 2000.

P. 87, para. 2, ll. 1-4. Given our current understanding of additive mortality, it is highly unlikely that harvest could be additive under the current permit system in CA.

P. 87, para. 2, ll. 6-9. There are a number of problems with Gibson et al. 2011, including the fact that changes in lek counts of the magnitude they report cannot have resulted from the harvest rates reported by CDFW. Gibson et al. do not address the effect of harvest on mortality, rather changes in lek counts.

P. 88, para. 1. This statement appears to represent a substantial degree of arm-waving and does not strengthen the document. What are the precise mechanisms by which humans have a greater impact in low productivity? Do these systems really have reduced resiliency?

P. 89, para. 2, l. 1. few, data are plural.

P.90, para. 4. Need a summary for the entire section on recreation. In my view, lack of data make interpretation of these effects difficult and speculative.

P. 91, para. 1, ll. 4-7. I note that mortality reported by Service is greater than that due to harvest.

P. 91, para. 2, ll. 5-6. Now Gibson et al. 2013.

P. 91, para. 4, ll. 8-10. These are not really confounding factors, i.e., correlated with visitation, rather they are additive factors that could increase variance in visitation effects among studies.

P. 92, para 2., ll. 7-9. Blomberg has analyzed data from NV and found the same increase in mortality due to GPS transmitters. In this analysis traditional necklaces were compared to GPS transmitters.

P. 95, para. 2, ll. 1-2. Newer assessments of the timing of mortality make this statement inaccurate. Mortality rates increase in Aug-October, which coincides with timing of WNV.

P. 98, para. 3, l. 3. Add Blomberg et al. 2013.

P. 100, para 1, ll. 3-5. Connelly et al. (2000) cannot be used to estimate mortality rates because they did not base their calculations on number alive in a given season, but rather the number initially marked in spring. Consequently, causes of mortality later in the year are overrepresented because a smaller proportion of the sample was still alive.

P. 101, para. 1, ll. 9-13. I think this hypothesis is based more on opinion than on data. I don't know of any studies that have studied predation risk across a range of different quality habitats.

P. 102, para. 2, l. 1. "is" rather than "and".

P. 102, para. 2, l. 2. Grass cover results are less well supported in the bi-state (Kolada et al. 2009) and Nevada (Gibson et al. 2013, Falcon-Gondor report).

P. 102, para. 2, ll. 4-5. I'm not sure there are references to support the statement about hen survival.

P. 103, last para. L. 17. Not relevant given more recent work. I would note that most SG studies before this date were poorly designed and data improperly analyzed.

P. 106, para. 2, last sentence. Several studies (e.g., Blomberg et al. 2012), including Garton's work, suggest that populations can increase fairly rapidly when conditions are good.

P. 107, ll. 1-2. While movements are probably adequate to maintain genetic diversity, they are almost certainly inadequate for demographic rescue in a metapopulation sense.

P. 107, para. 3, last sentence. I believe such estimates have since been disputed and populations must be much smaller for there to be a genetic consequence.

P. 110, para. 2, ll. 7-11. This statement is too strong.

P. 110, para. 3. There are numerous examples of populations of conservation concern that persist for years at numbers well below those predicted to be required for persistence. These examples call into question the theory. Small population size is certainly of concern, primarily related to stochastic demographic events, but I don't believe the theory is mature enough to quantitatively predict minimum viable population size.

P. 114, para. 2, ll. 16-17. This statement requires a citation of some sort.

P. 119, para. 3, last sentence. Is there an assessment of how effective it's been?

P. 122, para. 2, last sentence. Isn't lack of information a problem in and of itself? Is it possible to use the allotments where data do exist as a measure of how well BLM is doing in general?

P. 123, para. 3, last sentence. This is an important point.

P. 123, last para. A substantial proportion of nesting habitat and most late brood habitat is more than 3 miles from most leks in Eureka County.

P. 126, para. 3, last sentence. Important.

P. 127, last para., ll. 3-5. Poor seed dispersal is also a major limitation.

P. 129, Infrastructure-Roads. Disturbance associated with traffic and other human activity on roads can negatively impact sage-grouse. Data from Eureka County suggests that the linear habitat disturbance by itself has negative impacts on nest success. Closing roads to vehicles won't address this problem.

P. 129, Grazing-livestock, ll. 8-11. These are excellent steps. What fraction of total meadows do these 12 represent?

P. 129, Grazing-Wild Horses. I'm not familiar with the legal aspects of credit for horse management but I'm not convinced that Appropriate Management Levels are adequate for sage-grouse conservation because of the impacts of horses on meadows.

P. 130, Wildfire Fuel Reduction and Rehabilitation, para 1. Some fuels reduction (thinning of shrubs) may actually be detrimental to sage-grouse who prefer dense shrubs for nesting and who experience higher nest survival in dense shrubs.

P. 131, ll. 1-2. These efforts are critical. I wonder what proportion of total meadow habitat these represent.

P. 133, Threat Reduction. There is no explicit mention of meadow habitats, which are most limiting to SG throughout the remainder of NV.

P. 135, para 2, ll.2-4 . I'm not sure these theoretical minimum population sizes are very well substantiated by data.

P. 135, para. 2, last sentence. The preceding couple of sentences are pretty tough to decipher.

P. 135, last para., ll. 8-10. Connectivity is probably adequate to maintain gene flow but not demographic rescue.

P. 137, para 2, last sentence. Are meadow habitats in this PMU in good condition?

P. 139, para. 1, ll. 6-12. Late brood habitat is most limiting in most SG populations. Loss of access to this habitat, by itself, can lead to local extirpation.

P. 139, para. 2, ll. 3-5. We have not seen avoidance of either roads or transmission lines in Eureka County, although both negatively impact demographic rates.

P. 139, para. 2, ll. 13-17. An important limitation of Wisdom (and all other single variable regressions) is they do not account for the nonrandom association between environmental variables (slope, aspect, elevation, vegetation) and location of transmission lines or roads. Extirpation may be related to these other variables.

P. 141, para. 2, ll. 7-11. As with development of private lands, degradation of meadows, can have a population impact on local SG, disproportionate to the area impacted. What proportion of meadows in the bi-State are meeting standards?

P. 142, para. 1, ll. 7-10. Some experts (Peter Weisberg and Robin Tausch) believe that expansion of PJ is slowing or has stopped, associated with a drier climate, in other parts of the Great Basin. This hypothesis should be considered for the Bi-State area.

P. 143, para. 3, ll. 5-7. This statement needs a citation.

P. 143, para. 3, Last sentence. This statement needs a citation.

P. 144, para. 2, ll. 9-10. See above re: Weisberg and Tausch.

P. 145, para. 2, ll. 1-2. This sort of fragmentation is not well documented for SG.